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Behaviorism and the Stages of Scientific Activity

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Following from an earlier analysis by B. F. Skinner, the present article suggests that the verbal processes in science may usefully be viewed as following a three-stage progression. This progression starts with (a) identification of basic data, then moves to (b) description of relations among those data, and ultimately concludes with (c) the deployment of higher order concepts in statements about organizations of data. The article emphasizes the importance of viewing theory and explanation as examples of verbal processes at the later stages, guided by the stimulus control from the earlier stages. The article further suggests that many theories and explanations in traditional psychology often take a form that appears to be from the later stages. However, adequate activity at the earlier stages has not preceded those theories and explanations. They therefore do not have the benefit of suitable stimulus control from the earlier stages. Rather, they reflect some degree of stimulus control by many mentalistic assumptions about causal entities and relations. Ultimately, traditional theories and explanations influenced by mentalistic assumptions occasion less effective interaction with natural events (e.g., through prediction and control) than they might otherwise.

Key words: nature of science, hypothetico-deductive practices, contemplative ideal, technological ideal, pragmatism, prediction and control, interpretation

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Some years ago, Day (1969) suggested that "Science is at heart either the behavior of scientists or the artifacts of such activity" (pp. 318–319). Presumably, much of that behavior is verbal, and the artifacts in question are verbal products. Indeed, the common terms associated with doing science, like theorizing and explaining, imply instances of verbal behavior and verbal products. In this regard, readers may recall that chapter 18 of Skinner's (1957) landmark book *Verbal Behavior* is titled "Logical and Scientific Verbal Behavior." Accordingly, an analysis of science emphasizes an analysis of the underlying verbal processes as they have played out over time. The present article explores some implications of this position.

The present article draws on themes in other work by the author, and includes revised portions of that work.

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THE CHARACTER OF SCIENTIFIC ACTIVITY

The Influence of Ernst Mach on Radical Behaviorism

As described elsewhere, the Austrian mathematician, physicist, and philosopher of science Ernst Mach (1838–1916) strongly influenced Skinner's (1904–1990) intellectual development (Marr, 2003; Moore, 2005; Moxley, 2005; Skinner, 1967, 1979; Smith, 1986, 1995). For example, Skinner (1953) endorsed Mach's position that the first laws and theories of a science were probably rules developed by artisans and craftsmen who worked in a given area. As these individuals interacted with nature, they developed skilled repertoires. Descriptions of the effects brought about by relevant practices were then codified in the form of verbal statements that functioned as verbal stimuli, the purpose of which was to occasion effective action, if only among subordinates. The verbal statements, often taking the form of maxims or other informal expressions (e.g., "rules of thumb"), supplemented or replaced private or idiosyncratic forms of stimulus control. The verbal stimuli became public property, and were transmitted as part of the culture, enabling others to behave effectively.

Levels of Scientific Activity

However, science progressed beyond these lower level activities to develop higher order statements and concepts. A relevant passage from Skinner's writing is as follows:

> [Science] is a search for order, for uniformities, for lawful relations among the events in nature. It begins, as we all begin, by observing single episodes, but it quickly passes on to the general rule, to scientific law. … As Ernst Mach showed in tracing the history of the science of mechanics, the earliest laws of science were probably the rules used by craftsmen and artisans in training apprentices. … In a later stage science advances from the collection of rules or laws to larger systematic arrangements. Not only does it make statements about the world, it makes statements about statements. (Skinner, 1953, pp. 13–14)

Many scientific laws and theories therefore specify the relation between certain classes of responses and their consequences. In this regard, scientific laws and theories are not statements that are obeyed by Nature. Rather, scientific laws and theories are statements that exert discriminative control over the behavior of individuals who need to deal effectively with nature. The following passage from Skinner's writings gives further evidence of the general pragmatic orientation in a radical behaviorism concerned with organizing observations and facilitating desired outcomes:

> Scientific laws also specify or imply responses and their consequences. They are not, of course, obeyed by nature but by men who deal effectively with nature. The formula s = ½ gt does not govern the behavior of falling bodies, it governs those who correctly predict the position of falling bodies at given times. … As a culture produces maxims, laws, grammar, and science, its members find it easier to behave effectively without direct or prolonged contact with the contingencies of reinforcement thus formulated. … Science is in large part a direct analysis of the reinforcing systems found in nature; it is concerned with facilitating the behavior which is reinforced by them. … The point of science … is to analyze the contingencies of reinforcement found in nature and to formulate rules or laws which make it unnecessary to be exposed to them in order to behave appropriately. (Skinner, 1969, pp. 141, 143, 166)

Prediction and Control

Often science is said to be concerned with prediction and control. For example, James (1892) argued that, "All natural sciences aim at practical prediction and control" (p. 148). Subsequently, Watson (1913) argued that, "the theoretical goal [of psychology as the behaviorist views it] is the prediction and control of behavior" (p. 158). From the pragmatic perspective of a radical behaviorism, predictions about natural events are important for practical action concerning those events. Skinner (1953) commented as follows:

> The scientific "system," like the law, is designed to enable us to handle a subject matter more efficiently. What we call the scientific conception of a thing is not passive knowledge. Science is not concerned with contemplation. When we have discovered the laws which govern a part of the world about us, we are then ready to deal effectively with that part of the world. By predicting the occurrence of an event we are able to prepare for it. By arranging conditions in ways specified by the laws of a system, we not only predict, we control: we "cause" an event to occur or to assume certain characteristics. (pp. 13–14)

Prediction is important then as a guide by which to secure reinforcers from nature. When we can actually control antecedent circumstances, we can intervene or manipulate to produce desired ends. When we cannot actually control antecedent circumstances, we can nevertheless take action that results in desired ends. We obviously cannot intervene or manipulate the movement of the stars or planets, but by studying their movements we can gauge the seasons and when we can plant crops to produce a bountiful harvest.

STAGES OF THEORY BUILDING

Theories are traditionally regarded as an important feature of scientific behavior. In an important article, Skinner (1947/1972) elaborated a Machian line of reasoning as he more explicitly outlined three important stages in the development of theorizing:

> The first step in building a theory is to identify the basic data. … Since we have not clearly identified the significant data of a science of behavior, we do not arrive well prepared at the second stage of theory building, at which we are to express relations among data. … A weakness at the first stage of theory construction cannot be corrected at the second. … This step—at the third stage in theory building—can be exemplified by a simple example from the science of mechanics. Galileo, with the help of his predecessors, began by restricting himself to a limited set of data. He proposed to deal with the positions of bodies at given times, rather than with their color or hardness or size. This decision, characteristic of the first stage in building a theory, was not so easy as it seems to us today. Galileo then proceeded to demonstrate the relation between position and time—the position of a ball on an inclined plan and the time which had elapsed since its release. Something else then emerged—namely, the concept of acceleration. Later, as other facts were added, other concepts appeared—mass, force, and so on. Third-stage concepts of this sort are something more than the second-stage laws from which they are derived. They are peculiarly the product of theory-making in the best sense, and they cannot be arrived at through any other process. There are few, if any, clear-cut examples of comparable third-stage concepts in psychology, and the crystal ball grows cloudy. … When it is possible to complete a theoretical analysis at this stage, concepts of this sort will be put in good scientific order. … From all of this should emerge a new conception of the individual as the locus of a system of variables. … A proper theory must be able to represent the multiplicity of response systems. It must do something more: it must abolish the conception of the individual as a doer, as an originator of action. This is a difficult task. The simple fact is that psychologists have never made a thoroughgoing renunciation of the inner man. He is surreptitiously appealed to from time to time in all our thinking, especially when we are faced with a bit of behavior which is difficult to explain otherwise. (pp. 305–308)

In light of this series, it may be useful to view the stages as modes on a continuum, rather than as discrete, nonoverlapping activities. In any case, we start by noting that scientific statements are derived from contact with events and are ultimately applicable to events. The fundamental concern with events ensures that scientific activity is anchored to human behavior. Much of science begins by analyzing events and identifying the constituent participants of events, for example, as classes of variables or factors. Once identified, the participants in events are then available for prediction and control, and for abstraction, integration, and generalization into further statements with higher order concepts that characterize how the variables and factors relate to each other in a system.

With regard to a science of behavior, an example of the first stage of scientific activity is the identification of parameters of reinforcement and responding (perhaps dealt with as frequency over time or as probability) as basic independent and dependent variables. An example of the second stage is the identification of functional relations between the parameters of reinforcement and responding. An example of the third stage of scientific activity is organizing the variables, factors, and relations identified in the first and second stages into a comprehensive system. Such a system would deploy higher order concepts to yield an economical description of the facts so organized, reduced to a minimal number of terms.

IS RADICAL BEHAVIORISM ATHEORETICAL?

Traditional psychologists sometimes suggest that radical behaviorism is "atheoretical," usually as a consequence of Skinner's (1950) article that rhetorically questioned the assumption that all research activity had to test theories according to the hypothetico-deductive method in order to be considered legitimate. Often the claim is that some organizing framework is necessary to properly collect and evaluate experimental data, thereby giving meaning to the process. For example, Kant held that observation is blind unless guided by theory. Similarly, Poincaré (1913) stated that,

> Experiment is the sole source of truth. It alone can teach us anything new; it alone can give us certainty. … Merely to observe is not enough. We must use our observations, and to do that we must generalize. … The scientist must set in order. Science is built up with facts as a house is with stones. But a collection of facts is no more a science than a heap of stones is a house. (p. 101)

Radical Behaviorist Replies to Claims That It Is Atheoretical

Radical behaviorists actually have at least two replies to the traditional claim that it is atheoretical. First, radical behaviorists reply that first- and second-stage activities need not be carried out by testing a theory. They can "proceed in a rather Baconian fashion" (Skinner, 1969, p. 82) by manipulating variables "selected for study through a commonsense exploration of the field" (Skinner in Catania & Harnad, 1988, p. 101). Indeed, with his tongue firmly planted in his cheek, Skinner (1956) once pointed out that when they conduct research, researchers may have to heed five important principles that are not often formally recognized by scientific methodologists: (a) When you run onto something interesting, drop everything else and study it. (b) Some ways of doing research are easier than others. (c) Some people are lucky. (d) Apparatuses sometimes break down. (e) Serendipity—you may find one thing while looking for something else. In a more serious vein, we may note that it may even be wasteful to conduct research at these stages that presumes to test a theory. The appropriate foundation needs to be established before potentially useful third-stage concepts will appear and need to be evaluated.

Second, Skinner studied Poincaré extensively while he was in graduate school, and was much influenced by Poincaré's writings (e.g., Skinner, 1979, pp. 66, 83). For instance, consistent with the passage above from Poincaré, Skinner (1947/1972) stated the following:

> But the cataloguing of functional relationships is not enough. These are the basic facts of a science, but the accumulation of facts is not science itself. There are scientific handbooks containing hundreds of thousands of isolated facts—perhaps the most concentrated knowledge in existence—but these are not science. Physics is more than a collection of physical constants, just as chemistry is more than a statement of the properties of elements and compounds. … Behavior can only be satisfactorily understood by going beyond the facts themselves. What is needed is a theory of behavior. … Theories are based upon facts; they are statements about organizations of facts. … With proper operational care, they need be nothing more than that. But they have a wider generality which transcends particular facts and gives them a wider usefulness. … Experimental psychology is properly and inevitably committed to the construction of a theory of behavior. A theory is essential to the scientific understanding of behavior as a subject matter. (pp. 301–302)

Clearly, then, a radical behaviorism has always been intimately concerned with developing a theory. It is just that, for radical behaviorism, theories come about as a result of the three-stage progression described above.

Radical Behaviorism and Theories

Skinner further described his own position on theories in the following way:

> In 1950 I asked the question, Are theories of learning necessary? and suggested the answer was no. … Fortunately, I had defined my terms. The word *theory* was to mean "any explanation of an observed fact which appeals to events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions"—events, for example, in the real nervous system, the conceptual system, or the mind. I argued that theories of this sort had

not stimulated good research on learning and that they misrepresented the facts to be accounted for, gave false assurances about the state of our knowledge, and led to the continued use of methods which should be abandoned. (Skinner in Catania & Harnad, 1988, pp. 101–102)

In conjunction with the points above, Skinner stated,

> This does not exclude the possibility of theory in another sense. Beyond the collection of uniform relationships lies the need for a formal representation of the data reduced to a minimal number of terms. A theoretical construction may yield greater generality than any assemblage of facts. But such a construction will not refer to another dimensional system and will not, therefore, fall within our present definition. It will not stand in the way of our search for functional relations because it will arise only after relevant variables have been found and studied. Though it may be difficult to understand, it will not be easily misunderstood, and it will have none of the objectionable effects of the theories here considered. We do not seem to be ready for theory in this sense. At the moment we make little effective use of empirical, let alone rational, equations. A few of the present curves could have been fairly closely fitted. But the most elementary preliminary research shows that there are many relevant variables, and until their importance has been experimentally determined, an equation which allows for them will have so many arbitrary constants that a good fit will be a matter of course and cause for very little satisfaction. (Skinner in Catania & Harnad, 1988, p. 101)

Recapitulation

In sum, radical behaviorists suggest that many traditional theories in psychology have not gone through anything remotely resembling a developmental process such as outlined above, three stages or otherwise. The important consideration is that verbal processes at the earlier stages establish a large degree of stimulus control over verbal processes at the later stages. At issue is whether traditional psychologists recognize that in the absence of this kind of stimulus control, their "theoretical" verbal behavior can be controlled to a large extent by mischievous factors that are cherished for irrelevant and extraneous reasons. Their verbal responses can be conformist, that is, the product of many mentalistic if not dualistic factors popular in the culture. In addition, the verbal responses can consist of many socially approved but unfortunate metaphorical extensions. The verbal responses can end up appealing to other dimensions at the first and second stages, and consequently get off track (Moore, 2008a, pp. 273–274). The result is mentalism. Because of these mentalistic influences, the stimulus control in many cases over what are hailed as advanced third-stage verbal activities is suspect. Theory testing according to a hypothetico-deductive process does not necessarily correct these problems.

MORE ON THEORIZING

Theories and Cause-and-Effect Relations

An important feature of Skinner's analysis of theories, explanations, and scientific verbal behavior is that statements of facts that identify cause-and-effect relations may well be conspicuous at the first and second stages of theory development. For example, Skinner (1964) stated "When I said 'explanation,' I simply meant the causal account. An explanation is the demonstration of a functional relationship between behavior and manipulable or controllable variables" (p. 102).

Interestingly, Russell (1932) noted that cause-and-effect statements may turn out to be absent from certain scientific renderings: "All philosophers, of every school, imagine that causation is one of the fundamental axioms or postulates of science, yet, oddly enough, in advanced sciences such as gravitational astronomy, the word 'cause' never occurs" (p. 180). The seeming inconsistency between Skinner and Russell can be readily reconciled by recognizing that the terms *cause* and *effect* are typically absorbed into higher order third-stage statements taken as theories through the verbal processes inherent in their development. Thus, causal analysis lies at the heart of science, although the final statements in a theory may not have the form of cause and effect (Moore, 2000).

A parallel with other sciences may be informative. Chemistry identified basic data, such as those associated with the various elements in compounds, using the methods of the natural sciences. This activity was at the first stage. Then basic relations were studied, such as what amounts of elements were in the compound and how much energy might be required to cause the compounds. This activity was at the second stage. Then statements about organizations of data were made. A periodic table of the elements was developed, with protons and neutrons in the nucleus of an atom of the element, electrons in shells defined by various energy levels, and so on. This activity was at the third stage.

Theories, Systems, and Reinforcers for Higher Order Scientific Activity

Readers may recall that chapter 1 in Skinner's (1938) *The Behavior of Organisms* is titled "A System of Behavior." In it Skinner said "I am interested, first, in setting up a system of behavior in terms of which the facts of a science may be stated and, second, in testing the system experimentally at some of its more important points" (p. 5). Thus, Skinner began to follow the three-stage progression early in his own research career. Behavioral data such as rate or probability of responding were identified at Stage 1, rather than introspective statements about mental life. The effects of various manipulations and operations on responding were investigated and formulated in the second stage. Skinner (1938, pp. 12 ff.) further talked about second-stage activities in a discussion of static and dynamic laws of the reflex, following from Sherrington (1906) and to some extent Pavlov. At the third stage, the data and relations were brought together and applied to larger contexts. An example of advanced third-stage activity is much of the latter half of Skinner (1953), as well as Skinner (1969).

In one volume of his autobiography, Skinner (1979) reviewed his own scientific behavior from the point of view of contingencies:

> Was not confirmation the be-all and end-all of science? It was a question concerning my own behavior, and I thought I had an answer: "What is the motivational substitute for thing-confirmation? Pretty important in teaching method to graduate students. Resulting *order* instead of *confirmation*?" My reinforcers were the discovery of uniformities, the ordering of confusing data, the resolution of puzzlement. (p. 282)

Here, Skinner was presumably reflecting on the reinforcers for third-stage activity, for bringing together the data in a meaningful and systematic way, just as in Poincaré (1913) and Skinner (1947/1972).

Theories and Mentalism

As alluded to above, Skinner's statements about theories and explanations that appeal to causal processes in "different dimensions" (e.g., in the real nervous system, a conceptual nervous system, or the mind) are concerned with mentalism. More specifically, mentalism is the appeal to acts, states, mechanisms, processes, entities, structures, and the like, assumed to be from a dimensional system that differs from the one in which behavior takes place, as causally effective antecedents in an explanation of behavior. Radical behaviorism is concerned about the dimensions of theories and explanations when they include elements that are not expressed in the same terms and cannot be confirmed with the same methods of observation and analysis as the facts they are said to address (e.g., Catania & Harnad, 1988, p. 88). For example, the theory or explanation might be couched in metaphors like "information processing," or buckets that fill up but then leak, or springs that wind up but then unwind, or even supposed neural or physiological properties that have never been observed but are "inferred" from behavior. Similarly, the everyday language of folk psychology, which attributes existential, explanatory status to wishes, wants, desires, and intentions as mental things different from behavioral things, is mentalism. In short, Skinner's definition raises concerns about theories and explanations that appeal to "internal" or "inner" causes and dimensions.

One common sense of *internal* in traditional psychology, perhaps even one of its defining characteristics, is that of the mental or cognitive. Radical behaviorism is concerned about talk of mental causes and dimensions because such talk is a product of nonscientific influences. Some examples of these influences are (a) common linguistic practices of converting adjectives and adverbs into nouns (*reification*, recognizing of course that words can neither literally create nor change the nature of the things talked about); (b) unfortunate and ultimately mischievous metaphors; and (c) outright dualistic assumptions. Concerns with supposed mental causes ultimately divert more effective analyses in terms of causal relations in the one dimension in which behavior takes place. As Skinner (1938) put it in a very early discussion, his view was that at that time, a science of behavior

> appeared in the form of a remodeled psychology with ill-conceived evidences of its earlier frame. It accepted an organization of data based on ancient concepts which were not an essential part of its own structure. It inherited a language so infused with metaphor and implication that it was frequently impossible merely to talk about behavior without raising the ghosts of dead systems. Worst of all, it carried on the practice of seeking solution for the problems of behavior elsewhere than in the behavior itself. (pp. 4–5)

Skinner saw his project as redefining psychology as the science of behavior, in which behavior was taken as a subject matter in its own right, as Watson (1878–1958) had earlier envisioned it. Skinner chafed at the delays in getting there, even later in his career:

> As a philosophy of a science of behavior, behaviorism calls for probably the most drastic change ever proposed in our way of thinking about man. It is almost literally a matter of turning the explanation of behavior inside out. … I contend that behavioral science has not made a greater contribution just because it is not very behavioristic. (Skinner, 1974, pp. 256–257)
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> We have not advanced more rapidly to the methods and instruments needed in the study of behavior precisely because of the diverting preoccupation with a supposed or real inner life. … (Skinner, 1978, p. 77)

Often theories are the vehicle that perpetuates mentalism. Mentalistic theories superficially have the form of a third-stage verbal product, but have not gone through developmental verbal processes associated with the first two stages. As a result, mentalistic and cognitive theories are likely to be concerned with spurious data and relations and do not effectively occasion prediction and control. Indeed, Skinner put it even more strongly: "Cognitive science is the creation science of psychology, as it struggles to maintain the position of a mind or self" (Skinner, 1990, p. 1209), and "I think cognitive psychology is a great hoax and a fraud, and that goes for brain science, too" (Skinner in Goleman, 1987, p. Y18).

QUANTITATIVE ANALYSIS OF OPERANT CHOICE BEHAVIOR

The Generalized Matching Law

One prominent feature of contemporary research in the experimental analysis of behavior is a quantitative treatment known as the generalized matching law (GML; e.g., Baum, 1974, 1979). An important element of the GML is the exponent *a*, a parameter estimated after the fact in a data set that is said to describe sensitivity to reinforcement in the experimental setting. If a value of 1.0 for *a* yields the best description of the observed data, we say strict matching has occurred, but other values of *a* have been observed. Indeed, Davison and McCarthy (1988) suggested that the value of *a* that best describes the observed data in many studies actually tends to be less than 1.0, often close to .80. Moore (2008b) has recently called attention to the resemblance between the exponent *a* in the GML and the exponent *n* in Stevens's (1957) psychophysical law, said to describe sensitivity to stimuli in the modality being examined. Stevens, of course, was an influential member of the Harvard Psychology Department when much of the research and discussion related to what would eventually become the GML were taking place.

According to Grace (1996), "it should be emphasized that … the generalized matching law … is fundamentally a descriptive, molar model. Its fitted parameters provide a set of higher order dependent variables to guide research" (p. 376). Thus, the GML is not a causal law that specifies what caused some observed relation between independent and dependent variables. Rather, it uses free parameters to describe an observed relation between obtained reinforcement and responding in one or more already existing sets of data.

Prediction, Control, and the GML

What then about prediction and control? Ordinarily, we predict a dependent variable on the basis of a priori knowledge of an independent variable. In simple terms, we predict an effect from a cause. Predictions generated from the GML are not of this sort. To predict is to engage in verbal behavior under the control of some discriminative stimulus. The discriminative stimulus for the verbal product called the GML is explicitly identified as obtained reinforcement, not scheduled reinforcement. Thus, because the data have already been observed, and because the GML uses obtained rather than scheduled reinforcement frequency, there is no a priori independent variable in the ordinary sense of prediction. The effect has already happened, so there is no need to predict it. Thus, the GML can be contrasted with Skinner's (1953, pp. 13–14) passage cited earlier in this article, in which he emphasized the practical value of predicting—to prepare for some future state of affairs. To be sure, we can predict that the data can be fit post hoc to the basic form of the equation, given such free parameters as a for sensitivity that are estimated post hoc to secure a suitable fit. Similarly, we can predict the data by assuming that scheduled reinforcement frequency will be close to that obtained, which it often is, even though the GML is explicitly phrased in terms of obtained reinforcement.

Is the GML a Third-Stage Activity?

To use Skinner's framework, is it then possible to construe the GML as a third-stage activity (see Moore, 2008b, for discussion of this point)? For example, Killeen (1972) stated that "Viewed as a defining relation, matching deserves the label of 'law' just as does the defining relation between voltage, resistance, and current that we speak of as Ohm's law" (p. 492). Clearly, Ohm's law would be a third-stage activity. For present purposes, it is important to return to the point that third-stage statements come at the end of a developmental process that includes earlier cause-and-effect statements (e.g., in Stage 2), as Skinner described in his three-stages argument. At issue is whether comparable cause-and-effect statements have been forthcoming in much of the quantitative literature. The closest seems to be melioration (Herrnstein, 1997). Whereas others spoke of momentary maximizing, which would be a cause-and-effect principle, Herrnstein (pp. 68 ff.) disparaged it.

Causal Explanation, Mediation, and the GML

To be sure, one benefit of the GML is to show that behavior is ultimately an orderly subject matter that can be given a quantitative analysis, much as can be other subject matters in other sciences. Nevertheless, Moore (2008b) expressed concern that the GML is a form of explanation by instantiation, and thereby regresses into essentialism, as in Stevens's (1957) psychophysical law. The psychophysical law is based on assumptions of an S-O-R mediational model: A public, objective stimulus that can be agreed on is taken to produce a private, subjective sensation, which in turn is taken to produce public, objective behavior that can be agreed on, such as a verbal report ostensibly about the strength of the sensation. The inferred, subjective sensation is assumed to be the critical causal variable, because subjects are never directly in contact with the environment but only with the mediating organismic variable. However, it is private and cannot be part of the body of science if only because it cannot be agreed on. The experimental operations of the discrimination procedure and the accompanying mathematics are taken to produce agreement about the nature of the inferred, subjective sensation, thereby making the whole enterprise scientifically legitimate. According to Skinner (1969), "S. S. Stevens has applied Bridgman's principle [of operationism] to psychology, not to decide whether subjective events exist, but to determine the extent to which we can deal with them scientifically" (p. 227).

By substituting just a few terms for the different context, we can now say many of the same things about the GML and the extent to which it ultimately subscribes to an S-O-R mediational model. A public, objective variable like reinforcement that can be agreed on is taken to produce private, subjective "value," which in turn is taken to produce public, objective behavior that can be agreed on, such as the distribution of responding on a concurrent schedule. The inferred, subjective value is assumed to be the critical causal variable, because subjects are never directly in contact with the environment but only with the mediating organismic variable of subjective value. The experimental operations of the concurrent-schedules procedure and the accompanying mathematics are taken to produce agreement about the nature of inferred, subjective value, thereby making the whole enterprise scientifically legitimate. For both the psychophysical law and the GML, then, the mathematics are presumed to reflect the essential characteristics of the underlying subjective processes, which cannot be talked about directly because they are not publicly observable. Some passages from the literature of the operant quantitative analysis of behavior reflect the commitment to the mediational model:

> Behavior is often more easily analyzed in terms of relevant psychological dimensions, rather than arbitrary physical dimensions. (Killeen, 1968, p. 268)

> What is left out of equation 5a is a name for the concatenation of subjective scales. "Value" may be introduced as such a name. (Killeen, 1972, p. 491)

> In the context of some standardized concurrent design, the matching relation may be used as a formula for defining subjective scales of reinforcement. (Killeen, 1972, p. 492)

> The hypothesis that choice and timing should be mediated by a common representation of

reinforcer delay has been tested in studies using a modified concurrent chains procedure. (Kyonka & Grace, 2007, p. 394)

Of attendant concern is whether greater attention to causal processes would go a long way toward clarifying the value of activity in the quantitative analysis of behavior. The pragmatic spirit of Bacon and Mach calls for greater interest in direct contact with a subject matter than in words. In this regard, Ferster's (1978) comments in a review of Honig and Staddon's (1977) *Handbook of Operant Behavior* may well apply to the contemporary status of the quantitative analysis of behavior:

> One can speculate about the reinforcer maintaining the behavior of the writers of the various chapters of the *Handbook*—whether the reinforcer is the direct experience of altering the behavior of the experimental subject or the argument and discussion that becomes possible as a result of the empirical discovery. … For one experimenter, the validation of the experiment is the change in the behavior of the individual subject, guided by principle or instruction. For the other, the observations are merely a first step to secure the personal and social validation that occurs when the theory or conceptual scheme is argued. … The chapters of the *Handbook* span the full range of this continuum, but this reviewer found a preponderance of the latter, and I must confess a nostalgia for the former. (p. 348)

By way of contrast with Ferster, we note that researchers and theorists with interests in the quantitative analysis of behavior have often reported that they are more concerned with how to manipulate data than with behavior itself: "It would be well, therefore, to focus future investigations on the manipulations necessary to confirm the law, rather than on whether the law is true" (Rachlin, 1971, p. 251).

CONTEMPLATIVE VERSUS TECHNOLOGICAL IDEALS OF SCIENCE

The Influence of Francis Bacon

In a provocative article, Smith (1992) reviewed the influence of Francis Bacon (1561–1626) on scientific pragmatism and offered the following analysis, the pragmatic themes of which accord comfortably with a behavior-analytic approach to science reviewed above:

> Bacon's (1620/1960) epochal declaration that "human knowledge and human power meet in one" (p. 39)—one of the Baconian principles that Skinner (1983) said governed his own life—is no mere claim that contemplative knowledge can be put to human uses; rather it is the declaration of a different kind of knowing, in which the power of producing effects is not simply the by-product of knowledge, but rather the criterion of its soundness. … To understand nature's preferred forms, says Bacon (1620/1960), requires a "very diligent dissection and anatomy of the world" (p. 113) in order to reveal those "true and exquisite lines" by which underlying order is expressed in nature. … For Bacon, to know a cause is to have the ability to produce an effect. … Bacon spoke of two aspects of natural science. The first is what Bacon (1623/1937) called the "Inquisition of Causes," which involves "searching into the bowels of nature" to learn of its causal structure and preferred forms. The second is what Bacon called the "Production of Effects"—also referred to by him as "the Operative" aspect of science—which involves "shaping nature as on an anvil" (p. 413). But just as knowledge and power are ultimately one for Bacon, so too the inquisition of causes is fused with the production of effects. In Bacon's view, the search for causes, although sometimes aided by naturalistic observation, is best pursued through the experimental method—that is, the production and reproduction of effects. If we are to learn efficiently from nature, wrote Bacon (1620/1960), nature must be "forced out of her natural state, and squeezed and moulded," because "the nature of things betrays itself more readily under the vexations of art [i.e., of experiments] than in its natural freedom" (p. 25). For Bacon, the power to manipulate nature is thus the beginning and end of science: The beginning when nature is "squeezed and moulded" by experiment to reveal its order, and the end when scientific laws, construed as rules of operation, permit the shaping of nature as on an anvil for the improvement of the human condition. In taking the artisan as the model knower, Bacon elevated homo faber (the making human) over homo sapiens (the knowing human); just as the anthropologists give homo faber temporal priority over homo sapiens in the development of the species, so Bacon gave the human maker epistemological priority over the human knower (Perez-Ramos, 1988). (pp. 217–218)

Contemplative versus Technological Ideals of Science

Smith (1992) further noted that many historians and philosophers of science distinguish between two broad ideal types of science. The first is the contemplative ideal. Smith traced this point of view back to Aristotle. It seeks to "understand" events in the "natural world" and their causes. Its methods are based largely on passive observation. It emphasizes classification of natural phenomena and systematic description. It further argues that a full understanding of nature is attainable only through "theoretical" knowledge.

The second is the technological ideal. Smith (1992) traced this point of view back to Bacon, and perhaps also Mach. The technological ideal is predicated on practical, productive knowledge—how to control, make, and remake the world. Its methods are grounded in active experimentation, emphasizing hands-on manipulation of natural materials as well as experimental variables. Intervention in the course of nature is held to be especially revealing of natural processes, and the reformist bent implicit in the technological ideal has had a strong appeal in American culture. Nevertheless, those inclined toward the contemplative ideal regard the technological ideal as inferior because it involved merely imitations of nature that could never fully reproduce the effects of nature, much less supersede them.

Behaviorism and the Technological Ideal

Smith (1992) noted that at a general level, American behaviorism was consistent with the technological ideal. It was interventionist with a vengeance. An early figure in the history of American psychology that could be identified with this ideal is Jacques Loeb (1859–1924), who was a prominent faculty member at the University of Chicago during the late 19th and early 20th centuries. Loeb talked often of the engineering ideal in connection with human behavior, and Watson was one of the students at Chicago during Loeb's time there. Clearly the writings of Watson (e.g., those related to child raising) align well with the technological ideal.

The technological ideal flourished in many Western countries during the 19th and 20th centuries, and clearly contributed much to society in those countries, as standards of living, education, and health care improved on average. Of course, there were also longer term consequences, as measured in the degradation of the environment and alienation of workers. Smith (1992) reports that despite its benefits, the technological ideal has fallen from favor in recent years. Perhaps its decline is due to the excesses of the Protestant ethic and free-enterprise capitalism, or even postmodernism. Some sectors of society have often taken the stance that just because they can do something, they are entitled to do so. For example, if they have the technical capacity to use previously untrained workers to extract nonrenewable resources from the ground for the purpose of using other untrained workers to manufacture commercial goods, they should be allowed to do in the near term whatever is not expressly prohibited by law, regardless of longer term consequences.

As a canonical representative of 20th century American behaviorism, Skinner embraced the technological ideal in many respects. His approach to psychology was heavily influenced by Bacon and Mach (Moore, 2005). Readers may recall that previously in this article, we cited Skinner's (1953) argument that "Science is not concerned with contemplation" (p. 14). That Skinner should be tarred with the brush of technological excesses is not clear, however. From his early days, Skinner was strongly influenced by modernist trends of social melioration. He read much of Watson, for example, concerning child raising. From Bacon and Mach, he learned the value of controlling nature so that desired ends are realized, lest nature bring about undesired and unfortunate ends. His utopian novel *Walden Two* (1948) clearly showed the influence of Bacon. However, few if any technological excesses exist in Walden Two. To the contrary, cultural practices and daily life in Walden Two were deliberately structured to bring citizens into contact with their long-term consequences. Small is beautiful, but not because smallness promotes a desirable homogeneity. Rather, smallness facilitates direct contact with contingencies that underlie practices in daily life that contribute to the long-term survival of the culture (Skinner, 1987).

The Contemplative Ideal and the GML

Interestingly, Smith (1992) pointed out that some of the current activity in the quantitative analysis of operant choice behavior borders on the contemplative rather than technological:

> Finally, there is the possibility of the operant tradition moving back toward a contemplative model of knowledge. Operant psychologists have long scorned branches of psychology that by their very nature have little to say about how behavior can be directly changed—for example, the nativist traditions of ethology, developmental psychology, and Chomskian linguistics (Skinner, 1969). But it is just such fields that have enjoyed a resurgence as the technological model of science has fallen from grace. … Perhaps more important, some members of the operant tradition (e.g., Killeen, 1978 [sic; see Killeen, 1988], Staddon, 1983) have shown a renewed interest in the sort of mathematical models characteristic of contemplative science. (p. 222)

In partial recognition of Smith's argument, Moore's (2008b) recent analysis of the literature associated with the GML stressed that the importance of third-stage processes is that the resulting verbal products readily occasion practical, effective action in the form of prediction and control. It appears that in the absence of further assumptions, such as assuming scheduled reinforcement will approximate that obtained, the a priori predictive and practical value of the GML as it is currently formulated is limited. From the point of view of the present treatment, it is questionable to assume that we can arrive at the third stage without an adequate foundation in the first and second. In a larger sense, it is further questionable to assume that there actually is a cognitive or contemplative basis for a theory that differs from the end point of the effectiveness achieved by a technological basis. To view a theory as having a cognitive, contemplative basis apart from a technological basis is to actually assume that there is some metaphysical, essentialist, or Platonic order into which science taps at the third stage. Such an assumption constitutes mentalism.

INVESTIGATIVE METHODS IN SCIENCE

Why Do Scientists Do Science?

Moore (2008a, pp. 240 ff.) recently addressed the question of why scientists do science. His answer pointed out that for radical behaviorists, doing science is operant behavior. It is occasioned by particular antecedent circumstances, and it is maintained by particular outcomes. Moore then went on to cite Sidman (1960) regarding several of the antecedent circumstances that occasion scientific research: Scientists may want (a) to evaluate hypotheses, (b) to indulge their curiosity about nature, (c) to try out a new method or technique, (d) to establish the existence of a phenomenon, or (e) to explore the conditions under which a phenomenon occurs. The artifacts of research activity, such as scientific statements, are then available to guide the behavior of others with similar concerns. To be sure, some science is done to critically examine hypotheses, but not all is or even needs to be. As Skinner (1974) once put it, "The behavior of the scientist is often reconstructed by scientific methodologists within a logical framework of hypothesis, deduction, and the testing of theorems, but the reconstruction seldom represents the behavior of the scientist at work" (p. 343). Thus, the specific circumstances that cause scientists to do science in any given case are always going to be empirical matters.

Moore (2008a) then summarized the goals of a science of behavior according to radical behaviorism as follows (see also Catania & Harnad, 1988, p. 104):

1. To search for order, for lawfulness, for general relations in behavior.
2. To identify what methods are appropriate to the study of those relations.
3. To identify what aspects of behavior are significant.
4. To identify the variables of which changes in these aspects are a function.
5. To identify how the relations among behavior and its controlling variables are to be brought together in a system.
6. To start with a description of simple cases and collect facts, then advance tolarger systematic arrangements of those facts, where the arrangements include higher order concepts that aid in organizing the facts.
7. To identify the conditions under which such an analysis yields a technology of behavior and the issues that arise in its application.

These goals highlight that research is one way for experimenters to come under the control of variables and relations that participate in an event. By so doing, experimenters may better formulate and refine principles that inform the prediction and control of behavioral events. Research methods in a science are designed to promote effective scientific statements. They suggest manipulations that isolate the actions of relevant variables, so that their participation in events can be effectively understood (Johnston & Pennypacker, 1993; Sidman, 1960). By so doing, researchers are following Poincaré's (1913) earlier suggestion regarding experimentation. It is the bringing together of the data in a way that identifies how relations among behavior and its controlling variables may be effectively studied that constitutes an important aspect of third-stage activity. Perhaps the notion that even un-self-consciously mentalistic theories in traditional psychology make a heuristic contribution hints at this aspect of science.

Covering Law Model of Explanation

A traditionally important mode of explanation involves deductions from a "covering law" and entails the hypothetico-deductive method (Hempel & Oppenheim, 1948). According to this kind of explanation, an event is said to be explained when its description follows as a valid deduction in a logical argument in which at least one of the premises is a covering law and at least one of the other premises (if there is more than one other premise) is a statement of observed, factually specified antecedent conditions. Covering law explanations presumably get their name because the law "covers" the event to be explained, by subsuming the event under (e.g., as a logical consequence of) the law and antecedent conditions. In other words, a researcher or theorist elaborates an implication (i.e., a deduction) from a hypothesized covering law, or at least a lawlike generalization, and a statement of prevailing conditions. If the implication or deduction is supported by experimental data, then the law is presumed to be valid in some sense, and the status of the hypothesis is changed to a law.

Interestingly, Schoenfeld (1969) commented incisively on this very practice some years ago:

> Current emphasis upon deductive elaborations in psychology proceeds from the comforting, but I think mistaken, belief that the physical sciences owe their modern pragmatic successes to their constructional theoretical systems. Our students are taught that a theory begins with postulates or axioms that are unchallengeable; that these propositions contain terms that need no definition; that deductions (often claimed to be reducible to the classical syllogistic moods) are made within the self-contained system of propositions; that these deductions are then tested in the laboratory or field; and finally that if the empirical findings make it necessary, the propositions anterior to the empirical test are altered to conform with, and to generate, the new finding, but that otherwise congruent empirical findings may be declared to be "consistent with," though not to "prove," the system as it stands thus far. This sequence of practice is said to be beyond the power and legitimate scope of inductive procedures or inquiry. The latter are said to be simply incapable of rationalizing the practice because they force an inductive leap from particulars to universals. What is not often pointed out is the companion difficulty of deductive practice when it is described this way, namely, to say where the axioms or postulates come from in the first place. To reject this question as irrelevant or ad hominem, and to argue that only the ultimate correctness of the postulates is of interest, is to deny that human behavior is involved. It puts the origin of postulates into the sphere of disembodied whimsicality and mentalism, and thereby makes it impossible to instruct anyone in how to go about the business of science. This may perhaps satisfy some logicians, but it will not satisfy the true scientist. … Those same logicians, moreover, would not abide matching the same argument for the defense of induction; that is, the invalidity alleged of the leap is irrelevant, and the allegation ad hominem, and that only the final correctness of the leap is important. In truth, of course, the supposed opposition of deduction and induction cannot be found in the actual living work of scientists. They not only reduce to a single process in practice, but can be so reduced in verbal description as well. … It is, in point of fact, because the inductive generalization is universal in linguistic form, just as the postulate is, that "tests" of it are possible. It is not the form of the proposition that is at issue, but how the proposition has been arrived at. The inductive generalization openly declares itself to be based on previously ascertained facts, even if particular ones. But where does the postulate come from? It is plain silly to imagine any rational scientist actually doing what some have claimed he does or should do, or what he is praised for doing as a "deductivist-constructivist"; that is, close his eyes and reach into a grab bag of possible postulates, come up with whatever ones he chances upon, explore their logical consequences, put those consequences to experimental test, and then, if necessary, revise the postulates or go back to the grab bag for others. Such a view of scientific method anyone can have who wishes it. … That position, literally interpreted, not only removes the choice of postulates from connection with established knowledge, but it gives the fool equal rights with the scientist in the choice; it means that we yield any hope of acquiring new knowledge, since the chances of pulling a "good" postulate are vanishingly small because the contents of the grab bag are infinite in number; it means that even "good" postulates, being sentences of finite length, are doomed to be wrong when endlessly tested against an infinite world; it means that our purpose becomes one of proving propositions right or wrong, rather than of learning something about the world; and so on. This remoteness of origins and sources, their divorce from actual human behavior, is intended to give postulates unassailable rational status. But the intention does not square with reason, nor will it succeed in practice. … Not to be dismissed in this way are the questions of who is guessing how at what and why. (pp. 337–338)

Interpretation

A final important aspect of scientific activity on which we may touch is interpretation. *Interpretation* is the use of scientific terms and principles in talking about facts when too little is known to make prediction and control possible or when precise manipulation of antecedent circumstances is not feasible (Moore, 1998, p. 231). Two examples of interpretation are (a) the theory of evolution and (b) the theory of plate tectonics. These theories are interpretations of a vast number of facts, in one case about the origin of species and in the other about the nature of the earth's crust. They incorporate terms and principles taken from much more accessible material and from experimental analyses and their technological applications. The selectionist principles of variation, interaction with environment, and replication can be studied in the laboratory under controlled conditions, but their role in explanations of the evolution of species is interpretation. Similarly, the basic principles that govern the behavior of material under high pressure and high temperature can be studied in the laboratory under controlled conditions, but their role in explanations of the formation of surface features of the earth is interpretation (e.g., Skinner in Catania & Harnad, 1988, pp. 207–208).

Interpretation typically comes into play at the second and third stages of scientific activity. At these stages, there is again Poincaré's generalization from previously investigated to new cases, based on similar principles. In a sense, interpretation recapitulates Lyell's (1830) concept of uniformitarianism, which suggests that principles (e.g., of geology) that currently operate and are used to explain certain current phenomena presumably operated in the past and can be used to explain past phenomena. In other words, we assume that physical processes are uniform across time and place. In interpretation we assume that known principles can be applied to current situations, even though we have not performed an experimental analysis to demonstrate that they in fact do. In other words, we assume that behavioral processes are uniform across time and place.

SUMMARY AND CONCLUSIONS

Probably most researchers and theorists would maintain that their scientific statements are best regarded as useful descriptions of their observations of and interactions with nature rather than as metaphysical pronouncements about an ultimate reality. If so, then such statements presumably follow some pattern of development. The present article has laid out some possibilities for understanding one such pattern. The article has further contrasted statements related to this pattern with statements that are derived from mentalistic assumptions and are ultimately less effective.

In marked contrast to traditional views that are concerned with internal coherence of models or correspondence with mechanistic accounts, behavior analysis is robustly pragmatic. The value of its statements is measured by the extent to which they promote practical, effective action. In the final analysis, the question is what can we do to secure a better outcome of an event, by virtue of the statements we derive from doing research? In what ways do our statements facilitate interventions or manipulations that produce more reinforcing states of affairs? We surely do have physiological mechanisms inside our skin, and knowledge of these mechanisms can quite reasonably inform efforts to predict and control behavior. However, this sort of knowledge will come from direct investigation, rather than metaphorical inferences about acts, states, processes, structures, or entities supposedly in other dimensions. The mentalism of traditional views ultimately limits practical, effective action, by virtue of their concerns with other dimensions.

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